gamic, that these subjects can no longer be treated of *pari passu* with the structural, if this latter is to be brought up to the present state of knowledge in a work of the scope and design of the author's "Text-Book." This has determined Dr. Gray to enlarge the scope of his work, to retain the authorship of one volume, which is devoted to Morphology, Taxonomy, and Phytography, re-writing these throughout, to assign another upon Vegetable Histology and Physiology to his colleague, Prof. Goodall; a third, which will be an Introduction to Cryptogamous Plants, to another colleague, Prof. Farlow; and to complete the series by a fourth, from his own pen, on the Morphology and Classification of Flowering Plants, their Distribution, Products, &c.

Thus, when complete, we shall have from the most eminent botanical professors in the New World as comprehensive an introduction to the study of the Vegetable Kingdom as the nineteenth century is likely to produce.

OUR BOOK SHELF

Light and Heat; the Manifestations to our Senses of the Two Opposite Forces of Attraction and Repulsion in Nature. By Capt. W. Sedgwick, R.E. (London: Hodgson and Son, 1880.)

THE reviewer who says what he thinks is sometimes thought unkind. The author's paradoxes require no commentary but themselves to be duly appraised by scientific readers.

"The explanation of the fact that a spot of light is seen alike when pressure is applied to the outside of the eye, and when a single ray of light passes into the eye, is that the ray of light really makes itself manifest to our sense of vision by exerting a pull upon the retina of the eye . . . it follows, of course, that light is a pulling or an attractive force, and is therefore opposed to heat, which, as is well known, is a pushing or repulsive force." (Pp. 14 and 15.)

"Light consists of a large amount of the attractive force, mixed with a small amount of the repulsive force. Heat, on the other hand, consists of a large amount of the repulsive, with a small amount of the attractive force." (P. 28.)

"We have in the growth of plants and trees a beautiful exemplification of the action of heat and light as expansive and attractive forces. The young shoots are extended by the expansive power of heat, and then the attractive power of light comes into play" . . . (P. 38.)

"It may be objected that gravity cannot be the same force as light, because, if it were, it would be greater by day than by night." (P. 42.)

"There is ample evidence all about us to testify to the fact that light is an attractive force. Indeed *we ourselves bear witness to the fact by our fondness for fireworks and illuminations*" . . . (P. 38.)

"Light being the manifestation, in the free state, to our senses of the attractive or cohesive force . . . the fact that the production of light is made the first act in the creation of the world, in the account given us in the Book of Genesis, becomes intelligible." (P. 42.)

"I ask for no other favour, and for no mercy." (P. 3.)

We believe we have sufficiently complied with the gallant captain's request. S. P. T.

The Land and Freshwater Shells of the British Isles. By Richard Rimmer, F.L.S. (London: David Bogue, 1880.)

THIS unpretending and well-written volume is dedicated to the artisans, with many of whom, especially in the North of England, the subject is very popular. The dedication is qualified, viz.: "To those of my countrymen among the working classes who wisely employ their leisure hours in the pursuit of useful and elevating knowledge, with the hope that others among their ranks may be induced to forsake the paths of profitless and degrading dissipation." William Edward, the Banff shoemaker, is (thanks to Mr. Smiles) a celebrated example of the more intelligent workman; and we know of others who, however, "carent vate sacro." The book is very readable; it gives an excellent account of the habits of our land and freshwater mollusks, as well as of their various habitats, and it is not burdened with any synonymy or useless aliases. It is founded on Dr. Gwyn Jeffreys' "British Conchology." But the present work has a drawback. Eight out of the eleven plates give photographs of the shells, which are produced by the "Albertype" process; and the figures, especially of the smaller species, are so blurred or smudgy as to be almost undistinguishable. Plate X. is very good, representing magnified views of the British species of *Vertigo*. There is a useful glossary.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Novel Celestial Object

THE search for planetary nebulæ, described in NATURE, vol. xxii. p. 327, was continued for several evenings without revealing any new object, although it is estimated that the spectra of about 100,000 stars were examined. On the evening of August 28 an object entered the field which presented a faint continuous spectrum with a bright band near each end. Clouds interfered and barely permitted an identification with Oeltzen 17681, or a position in 1880 of R.A. 18h. 1m. 17s., Dec. – 21° 16′.

This object might be mistaken for a temporary star like that in Corona in 1863, and the bands assumed to correspond to the hydrogen lines C and F. This view is contraverted by the permanency of the object which was observed by Argelander in 1849, and at the Washington Observatory in 1848 and 1849. In all these cases its magnitude was estimated as 8, or very nearly its present brightness. No variation of light was detected between August 28 and September 1. The star Oeltzen 17648 precedes it very nearly a minute, and is only four minutes north, so that their light can be easily compared. As they are nearly equal, a slight change would be quickly recognised.

A further examination of the spectrum showed that the less refrangible band is near D, and the other between F and G. The images of both, but particularly of the second, are much elongated by the prism, showing that they are bands, and not lines. The limits of wave-length of the first band are approximately 5,800 and 5,850; those of the second, 4,670 and 4,730. A band at 5,400 and some other fainter bands were also suspected, but their existence is not certain.

The discovery of this object greatly increases the difficulty of distinguishing between a star and a planetary nebula. If the disk is used as a test, the first object described in the paper referred to above might easily be taken for a star. If we define a nebula by its spectrum of bright lines on a faint continuous spectrum, the object now under consideration becomes a nebula. Whether it is a mass of incandescent gas resembling a nebula in character, but not in constitution, or whether it is a star with a vast atmosphere of incandescent gas of a material not as yet known to us, are questions which cannot yet be decided.

Cambridge, U.S., September 2 EDWARD C. PICKERING

Experiments on the States of Matter

THE exploration of the new region which I have lately opened up has led to so many results with both scientific and technical bearings that I have been unable to leave this city for some time to attend any scientific meetings, and I would beg leave, with your kind permission, to make, through the medium of your valuable columns, a few remarks on some recent scientific work.

The quite independent confirmation of my discovery of the limit of the liquid state given in a letter in NATURE, vol. xxii. p. 435, by my old colleague, Dr. Carnelley, helps to dispel the idea of an intermediate state above the critical point, and confirms me in the use of the term "gas" for all fluids above their critical temperatures in speaking of the "solubility of solids in gases." The term vapour should only be applied to an aëriform fluid which by pressure alone can be reduced to the solid or liquid state, and above the critical temperature this cannot be done. As yet I have no evidence that vapours so defined are capable of dissolving solids, and this negative property may help to form a definition of that division of matter. As Dr. Carnelley does not mention the coincidence of our researches, perhaps you will permit me to quote from our respective papers. Dr. Carnelley says:—1. "In order to convert a gas into a liquid, the *temperature* must be *below* a certain point (termed by Andrews the *critical temperature* of the substance), otherwise no amount of pressure is capable of liquefying the gas."

As far back as May 24 I wrote ("On the State of Fluids at their Critical Temperatures," *Proc.* Roy. Soc. No. 205, 1880):—"The same results were obtained as before. When the temperature was below the critical point, the contents of the tube were liquid, and when over that temperature the reaction was always gaseous, notwithstanding the variations of pressure.

"I think we have in these experiments evidence that the liquid state ceases at the critical temperature, and that pressure will not materially alter the temperature at which the cohesion limit occurs." Dr. Carnelley will find the whole of my paper devoted to an experimental demonstration of what he has now deduced from his experiments. The paper was written with the title, "On the Cohesion Limit," but by the advice of Sir William Thomson, to whose great kindness in helping me with advice and information I am much indebted, I altered the title until I had the whole field explored. This I have since done, and have completely established the "cohesion limit" for all liquids—that for homogeneous liquids being an isotherm starting from the critical point. My paper being a very full one has taken much time and work, and the corrections for over a thousand experiments will take me some time yet. Prof. Stokes (whose kindly interest and encouragement have greatly lightened my labours) has been kept informed of my progress, and is cognisant of the work I have done in this direction. Dr. Carnelley's second conclusion is also very interesting, especially when applied to water; but surely we are not to understand that the solid ice was hot throughout, or that, if a thermometer had been imbedded in the ice, it would have risen. Although the vessel be red hot, the ice need never be allowed to melt, but made to pass directly into vapour, and yet its temperature remain 0° till it has been entirely volatilised.

I notice from your report of the British Association that Sir William Thomson calls attention to Cagniard de Latour's method of showing the critical state of a liquid by sealing the requisite proportion of liquid in a stout tube and heating it in a bath. It should not be forgotten that, although to Dr. Andrews undoubtedly belongs the credit of establishing the definite finish of the boiling-line and the apparent continuity of the liquid and gaseous states, to Baron Cagniard de Latour belongs the discovery of "l'état particulier" where the liquid state ends. Latour's method, although often used by Mr. Hogarth and myself, is not convenient for purposes of research. The method, your report goes on to say, was criticised by Prof. W. Ramsay, in what spirit we are not informed, but Dr. Ramsay added that he had found an apparatus in which a screw was employed to produce increase of pressure instead of using the expansion of the liquid itself. Dr. Ramsay, however, did not say whether the apparatus he had found was that invented by Mr. Hogarth and myself, and described by us in *Proc.* Roy. Soc., No. 201, 1880, in which india-rubber in a hollow cap is made by compression to yield a perfectly tight joint and to answer also for a screw when protected by a facing of leather. Dr. Ramsay visited my laboratory, and had the apparatus taken down and fitted up before his eyes, and with my permission had an apparatus made. The use of the compressed india-rubber for obtaining the requisite close fitting constitutes the important feature of my apparatus; the employment of iron for constructing the vessel enabling experimenters to dispense with the use of two liquids as in Andrews' apparatus—mercury being used alone.

I have made some little progress with the construction of vessels to withstand pressure at high temperatures, and I expect in a few weeks (when I have prospect of leisure) to carry my crystallisation experiments to a scientific if not commercial success.

J. B. HANNAY

Private Laboratory, Sword Street, Glasgow

Fascination

I EXPECTED some of your readers to refute the explanation of Mr. Stebbing on "Fascination." I see in NATURE, vol. xxii. p. 383 another paragraph which is not more to the purpose. Want of presence of mind and stupefaction are not fascination. In 1859 (twenty-one years ago) I followed in the rocks of Avon, close by the park of Fontainebleau, the fairy paths of Denecourt, when the approach of a storm induced me to leave the blue arrows, indicating the right path, for a short cut. I soon lost my way, and found myself in a maze of brambles and rocks, when I was startled by seeing on my left hand, at a distance of about ten yards, a snake, whose body lifted up from the ground at a height of about a yard, was swinging to and fro. I remained motionless, hesitating whether to advance or to retreat, but soon perceived that the snake did not mind me, but kept on maintaining its swinging motion, and some plaintive shrieks attracted my attention to a greenfinch perched on a branch of a young pine overhanging the snake, with his feathers ruffled, following by a nod of his head on each side of the branch the motions of the snake. He tottered, spread his wings, alighted on a lower branch, and so on until the last branch was reached. I then flung my stick at the snake, but the point of a rock broke it and the snake disappeared with the rapidity of an arrow. On approaching the spot, a real abode of vipers, which I did with the greatest precaution, knowing by observation that death may be the result of the bite of a viper, I saw the greenfinch on the ground agitated by convulsive and spasmodic motion, opening and shutting his eyes. I put him in my bosom to try the effect of heat, and hastened to reach the park of Fontainebleau. The little claws of the bird opening and shutting, perhaps as an effect of heat, made me think that he might perhaps be able to stand on my finger, and he did clutch it, and held on with spasmodic squeezes. In the park I got some water, and made him drink it. In short, he revived and finally flew off in the lime-trees of the park.

Now whilst following the motions of the snake and bird I experienced a singular sensation. I felt giddy; a squeezing like an iron hoop pressed in my temples, and the ground seemed to me to be heaving up and down. In fact the sensation was quite analogous to that experienced on a beginning of sea-sickness.

From these facts would it not seem probable that fascination is nothing more nor less than an extreme fatigue of the optic nerve, produced by a rapid gyratory motion of a shining object and resulting in a nervous attack and a coma? Curiosity rivets at first the attention of the bird, unconscious of any danger, and when giddiness warns him of his peril it is too late. The snake is as well aware of this as the *Lophius piscatorius* is of the effect of his membrane.

In this system the fact of the bird coming down from a higher to a lower branch would be explained by the supposition that, giddiness overtaking him, he opened instinctively his wings and clung to the next support that he found, the motion having partially removed the giddiness so as to enable him to hold fast.

Observe, that nothing hindered the bird from flying away, and that the snake being at most five feet long, could never have reached even the lowest branch.

Besides he could have no nest to protect, for in the rocks of Avon there is no water save rain-water in the hollows of the rocks, and this is not potable on account of microscopic leeches which people it, the instinct of birds teaching them to avoid it.

Jersey, August 29 CHATEL

P.S.—I inquired of Mr. Denecourt, "the sylvan of the forest," if he were aware of the existence of such large snakes in the forest, and he told me that he had only seen, in the "rocher Cuvier Chatillon," a snake about four and a half feet long, which he killed, but that even larger snakes had been seen in this very "rocher d'Avon" and in the "rocher St. Germain," but he thought that they were only "couleuvres" of a large size and quite inoffensive.

Meteor

ON the 19th inst., at 11.34 p.m. (within a minute of G.M.T.), I observed a large meteor in the east, towards which I happened to be looking, the sky being quite free from clouds, and clear.